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REMARKS

ON

THE PRESENT STATE OF THE THEORY
AND PRACTICE OF MEDICINE."

BEING

A REVIEW

OF

PROF. BENNETT'S PUBLISHED INTRODUCTORY LECTURE
(UNIVERSITY OF EDINBURGH, SESSION 1855-56).

BY

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REMARKS, &c.

(Read to the Hunterian Medical Society, January 14, 1856.)

MR PRESIDENT, AND GENTLEMEN—

IT appears to me that the principles on which this Society is constituted—the utmost rational freedom of discussion, and perfect professional equality of membership—afford good opportunities for the examination of received opinions, as well as the statement of independent views in medicine. I venture, therefore, to submit to you the following criticism of Dr Bennett's published Introductory Lecture; and I do so in the hope that any freedom, and it may even be severity of comment, into which from the nature of the undertaking I shall be led, will not be mistaken for the mere arrogance of the critic. It is because I conscientiously differ from Dr Bennett,—because I consider the scientific position he would assume for modern medicine unphilosophical and untenable—no less than because I think his lecture a narrow and illogical production—that I embrace this opportunity of indicating what I believe to be some of the barren speculations of modern medicine.

It is seldom that so good, or at least so available an opportunity offers; for the vast majority of medical writings, on account of their bulk, defy the passing criticism of an hour,—their size, if not their strength, doth laugh a sieve to scorn; and with what falls orally *ex cathedra*, I, of course, have no title to intermeddle. But when an Introductory Lecture is printed and indiscriminately sold, it becomes the

property of the professional public, and there can be no presumption—as there is no difficulty on account of its bulk—in subjecting it to the process of review, favourable or not favourable to its pretension, in the present instance, as a philosophic sketch of the scientific position and prospects of medicine.

I shall therefore treat the Lecture before us—as I am entitled to do—as if it were the production of some unknown Dr A., instead of Dr B., the Professor of Physiology in this University. We shall in fact adopt, for the nonce, an editorial style, and proceed as candidly and as fearlessly as if our noticee was intended for the pages of an ordinary Magazine.

Dr Bennett begins by drawing a distinction between the exact and inexact sciences, and by assigning to medicine a place among the latter; but in many succeeding parts of his lecture he loses sight of this important distinction, and in the most reckless manner subsequently speaks of medicine, now as if it were an exact science, again as a mere art. To those who have scanned much of our modern medical literature—not so much in the expectation of finding new facts there, as in the vain hope of lighting upon some tangible philosophy of medicine—this confusion of ideas, this constant shifting of the ground on which it is proposed to build a much talked of Rational Medicine, is not new or surprising. It is a confusion not confined to any writer in particular, but seemingly almost universal. Nor is it surprising; for when we come to analyse any of the definitions of medicine usually propounded, with one exception we find them untenable, and nearly useless as starting points.

To define medicine as an ‘inexact science,’ and in the next breath to speak of it as a ‘healing art,’ seems utterly inconsistent. For the first is much too general a definition, and will not bear analysis; the second is perhaps nearly correct; but if so, the scientific pretensions of medicine are at once disposed of, and it thenceforth takes its stand on simple observation and experience. Yet this is the kind of oscillating position which medicine, as represented by its literature, holds at this hour: it is scarcely possible to peruse a

medical work that professes to deal with principles, without encountering a lamentable logical confusion of this nature.

As we have said, Dr Bennett's definition of medicine—that of 'inexact science'—will not bear analysis: For it would be necessary to show,* of the separate branches of inquiry that make up the compound whole of medicine—that each of them separately is an inexact science also, or that they all agree together in constituting such a science. But this is not the case, for, according to Dr Bennett, one at least of the branches that go to make up the so-called inexact science of medicine—Chemistry, to wit—is itself an exact science; so that a part is greater than the whole, which is absurd. The definition is untenable also in the opposite direction, for besides chemistry, anatomy, physiology, *materia medica*, and botany, which are popularly known as the medical sciences, observation and experience are confessedly indispensable to the existence of medicine: but inasmuch as observation and experience are common alike to the savage and the physician, inasmuch as they are, in themselves, not sciences in any sense,—so it follows that, in as far as medicine rests on observation and experience, it is not in any necessary sense a science.†

* In the analysis of course,—not in the *definition*, as a certain sapient M.A. has taken it upon him to assert for us.

† A confused blending of philosophy with science still obtains in medical literature: witness the following passage from the able review of Dr Bennett's lecture and this pamphlet which appeared in the *Medical Times* for February 9:—"The relation of the laws of the mind to one another forms the foundation of the pure sciences (grammar, logic, morals, theology, and metaphysics), which have, therefore, all the certainty of that which is positive and absolute. In their discussion or study each step of our progress must be an act of the mind and of the mind alone."

Now the above classification is altogether scholastic, and has been growing obsolete ever since Mill and Lewes first pointed out the natural boundary which separates the entire field of mental philosophy (or *metaphysics*) from the domain of science. And, we apprehend, until this distinction is more generally recognised,—until what are above called the *pure sciences* cease to be so considered, the field of science will remain, as it still unfortunately is, indefinite and shifting. It is only when Philosophy and Metaphysics come to be received as translatable terms, that the word *science* will have an exact meaning, and a precise application to the investigations of medicine and other philosophico-experimental branches of knowledge. The very fact that each step

To what extent medicine rests on observation and experience is a further question. In our opinion it rests upon them primarily, mainly, almost entirely: but we have no direct excuse for defending such a thesis at present. It were not difficult to shew, however, that Dr Bennett virtually, though of course quite unintentionally, admits as much; —for, having defined medicine an inexact science, and given as the distinguishing characteristics of exact and inexact science, that the one does, and the other does not possess a primitive fact or law, “medicine”—says he, page 6—“in its present state, possesses *no* primitive fact.” Now if medicine possesses no primitive fact, on what, we might ask, can it fall back else than observation and experience?

Our lecturer, indeed, takes refuge in the future, as many have done before him, and ventures on the following prediction:—

“One day another Newton may arise, whose genius will furnish *our* science with *its* primitive fact. . . . Although it must be confessed that we have not yet arrived at such a happy consummation, it cannot be denied that we are making rapid strides towards it.”

On the contrary, we shall have occasion, before we have done, to suggest such doubts as amount to a decided denial of what Dr Bennett here assumes. In the meantime, we have only to observe that, from such a point of view a defi-

in the study of what used to be, and still sometimes are, called the *pure sciences* (logic, theology, &c.), is an act of the mind alone, of itself demonstrates that these branches of metaphysics *cannot* have “all the certainty of that which is precise and absolute.”

In the following passage from the introduction to Lewes’ “Biographical History of Philosophy,” the true nature (as we humbly think) of the distinction between the philosophy of mind and science is concisely drawn:—“Philosophy (metaphysical, remember!) aspires to the knowledge of *Essences* and *Causes*. Positive Science aspires only to the knowledge of *Laws*. The one pretends to discover *what things are* in themselves, apart from their appearances to sense, and whence they came. The other only wishes to discover their *modus operandi*; observing the constant *co-existences* and *successions* of phenomena amongst themselves, and generalizing them into some one *Law*. In other words, the one endeavours to compass the Impossible; the other knows the limits of human faculties and contents itself with the Possible. . . .

“Irreversible canon: whatever relates to the origin of things, *i.e.*, *causes*, and whatever relates to the existence of things, *per se*, *i.e.*, *essence*, are the proper objects of Philosophy, and are wholly eliminated from the aims and methods of Positive Science.”

nition of medicine is simply impossible. If it be an inexact science, characterised by possessing no primitive fact, we are not entitled to say—because it does not admit of proof—that it is ‘making rapid strides’ towards that very primitive fact the absence of which is essential to its definition.

The inevitable consequences of all attempts at a scientific definition of medicine—that is to say, confusion and a change of ground—are at length met by the admission that medicine is *an art*: Dr Bennett says, at page 9:—

“I trust that, in studying this, (Physiology,) as all other subjects, you will never lose sight of the important fact that you are medical students, and that as such, your ultimate object is to acquire an art, in other words a knowledge of all those means which are directed to the prolongation of life and cure of disease.”

This is quite intelligible. The practical result, the sum of the compound studies of medicine, is a *healing art*: as we have already hinted, no other definition is tenable. But it is inconsistent with the scientific position formerly claimed for medicine, and as if feeling it to be so, after insisting on the value of *theory* in the practice of the *art*, Dr Bennett proceeds to distinguish ‘carefully,’ as he says, “what is science and what is art,” thus (page 11):—

“We may consider then, science to be a collection of theories; art a body of rules. Science says, this is or is not; this is probable or improbable. Art says, do this, avoid that. The object of science is to discover facts and determine laws; the object of art is to accomplish an end, and to determine the means of effecting it. Science is inductive and reasons; art is imitative and exemplifies.”

In the above paragraph of definitions the lecturer riots in a loose use of terms, the exact and the inexact sciences being confounded: what he means may or may not be true, but what he says is undeniably incorrect. Are the mathematics, for example, a collection of theories?—is astronomy, founded on mathematics, a mere collection of theories? Moreover, science is not necessarily inductive, nor does it necessarily reason; on the contrary, exact or positive science is deductive, and it does not reason, it demonstrates. But this last kind of science repudiates medicine, and hence much of the confusion in which the champion of a would-be scientific medicine inevitably becomes involved.

What, however, Dr Bennett means—we take it—is, that there is an art as well as a science of medicine. But he means this evidently, not of medicine *as* medicine but, of some one or other of its branches or practical applications; and his subsequent “striking examples” of how far the one is dependent on the other—*i.e.*, art on science—partake, as might be expected, of the inconsequence attaching to the definitions they (*viz.*, the “striking examples”) are meant to illustrate. This we go on to show. Meanwhile let us grant (as it were) the lecturer’s dictum, that science is a collection of theories,—in other words, that *science* in medicine is synonymous with *theory*. We shall peradventure discover, by and by in following him farther, what is, according to him, the practical value to suffering humanity of such a collection of theories.

“Science,” he says, p. 11, “in numberless instances, has advanced beyond art; nay more, science herself has worked out all the details, and made art a mere slave to her commands. Thus it was that the theory of achromatism, worked out by Euler, led opticians to make perfect telescopes and microscopes.”

In this, the first of his ‘striking examples of how far art is directly dependent on science,’ Dr Bennett refers to the laws of optics *merely* to prove that science has “advanced beyond art;” he has not brought the illustration to bear on medicine in general, or physiology in particular. Useless in his hands, it may be of some account in ours; we shall show how it tells if honestly brought to bear on that branch of medical study with which he is more immediately connected—physiology.

The laws of optics refuse to be turned to physiological uses. The principles on which the human, and all other fully developed eyes appear to be constructed, are strictly scientific principles, and we are indebted to mathematics for their discovery, and their application to mechanical instruments, long before their explanation of the phenomena of vision was suspected.* But the instant we attempt to apply mathematical principles to the optical theories of physiology, we come to a stand-still. Of what value are the sev-

* Hence it is not correct, at least in this instance, to say, with Dr B., that “art for many ages preceded science.”

ral theories that have been advanced in explanation of the wonderful fact—That, while we possess the faculty of correct vision, the retina offers us an inverted image of every object we behold? Towards reconciling this apparent contradiction, the most various and mutually destructive theories—physical, metaphysical, and physiological—have been proposed by natural philosophers, as Sir David Brewster and others, on the one hand, and authoritative physiologists, as Muller, Volkmann, and Carpenter, on the other hand. The cause of single vision with the two eyes has given rise to equal, or greater variety of solution: at least five conflicting theories have been propounded by physiologists—Let us hear no more of the theories of optics in the interest of scientific medicine (so-called)! But again—

“Thus it was that Le Verrier and Adams, by calculations in their observatories in Paris and London, discovered a planet which they had never seen, but which, when looked for according to their directions from Stockholm and St Petersburg, was immediately proved to exist in fact, as it had previously been proved to exist in theory.”

Now, to say that this planet was discovered before it was seen, is to use very loose and unscientific language: It was merely *calculated* before it was seen; and although the calculation, being a mathematical one, was absolutely correct (and all mathematical calculations are necessarily either absolutely correct or absolutely incorrect), yet its observation, and nothing less, was the proof of its existence as a planet.* Of course it could not be “proved to exist in theory” before it was “proved to exist in fact;” its existence could only be rendered probable in theory, because *probability* is the utmost proof of which theory is capable,—a fact that stares Dr Bennett’s definition of science rather awkwardly in the face. He should have rendered this planetary example really “striking,” by furnishing a corresponding example of

* In the opinion of its discoverer (*as a planet*), Leverrier, Neptune is identical with the star of the eighth magnitude observed by Lalande on the 10th May 1795, and marked in his catalogue of the fixed stars. So far, therefore, from having been first proved to exist in theory, it was actually *seen* fifty years before it was dreamt of in theory: Dr Bennett, we suspect, was ignorant of this fact, or he would not have employed the illustration.

a medical theory founded on a mathematical calculation : he has not done so.

We pass over his third illustration, wherein "the electric telegraph, perfected in the closet of the man of science, flashes ready made on the astonished gaze of an admiring world," because it will again flash on *our* astonished gaze ; and now we come to another, and it is the last of the "striking examples" intended to show, in a medical direction, the dependence of art on science.

"The art of navigation," says he, p. 12, "is certain, because the science of astronomy on which it is based, admits of exact calculation ; and, in consequence, the tempest-tossed mariner, although in unknown seas, may, by his instruments, ascertain the exact spot his vessel occupies on the surface of the globe. In like manner, the only way of improving the art of medicine is to advance the science of physiology, and all that has been accomplished during the last fifty years has been brought about in this manner."

Nothing can well be more untrue than what is here assumed, to wit, that mathematics and astronomy on which navigation is based, and physiology, on which, for the nonsee, medicine is held *in like manner* to be based, are sciences of a similar kind ; and we might quite as fairly give to some other branch of medical study the isolation here claimed for physiology : we might quite as justly say, that "the only way of improving the art of medicine" is to advance chemistry or the *materia medica*.

We freely grant what the lecturer adds, viz., that the discovery of "the independent property of the nerves, the reflex functions of the nervous centres, &c., the laws regulating the development of the ovum, the significance of the sounds produced by the heart and lungs, and numerous other doctrines,"—some of them chemical, some mechanical, some anatomical, some derived from natural history or from simple observation—"have tended to improve the art of medicine." It by no means follows that they have rendered either physiology in particular, or medicine in general, a science similar to astronomy. We shall presently see, as far as Dr Bennett can let us, what these numerous doctrines have effected for the relief of suffering humanity.

"But let me descend," says he, p. 12, "from generalities to a few striking instances, of what a study of the theory of medicine has recently accomplished for the improvement of its practice."

We proceed now to examine these "striking instances," as we previously did the other "striking examples:—"

"From very ancient times the salt lakes of Fnsaro have been used as nurseries for young oysters. It has been calculated that each female oyster produces, on an average, about 100,000 young, which, when they escape from the shell, appear like a thick white cloud in the water. Each minute particle is at first furnished with cilia, by means of which it swims about till it finds a surface to which it attaches itself. If this cannot be found, the cilia soon fall off, and the young animal sinks to the bottom of the water, where it becomes a prey to the polypes which are fixed there. Now, at Fnsaro, it has been found that if fagots, or dried branches of trees, are driven into the sand, so as to present to this animated dust a surface to which it may attach itself, the minute particles cling to it, as a swarm of bees cling to any projecting substance. There they become fixed, grow rapidly, so that at the end of two or three years each minute ovum becomes an eatable oyster. Then the fagots or branches are pulled out, and people gather, at Fnsaro, a harvest of these shell-fish annually, in the same way that, in other places, they gather grapes."

..... It has long been known that the salmon fisheries in this country have been annually becoming less and less productive, so that what was formerly the food of the common people has become an article of luxury for the rich. Now, the experiments of Spallanzani demonstrated that a minute quantity of the male spermatic fluid, when diffused in water, would impregnate a multitude of ova obtained from the roe of the female frog, when sprinkled over them. This important fact has, within the last few years, been taken advantage of, with a view of producing artificial fecundation," &c.—Pp. 13, 14.

After reading the above, we may well ask what the researches of naturalists into the mode of propagation of the oyster—what the trite experiments of Spallanzani on the ova of frogs, have directly to do with the matter in hand. How these researches and experiments constitute a "striking instance of what a study of the theory of medicine has recently accomplished for the improvement of its practice," it puzzles us, we confess, to comprehend; and, with all his ingenuity, the lecturer himself cannot—at least he does not—tell. He says, indeed, at the beginning of the succeeding paragraph—

"Here then is an example of how physiology has been made useful in procuring food, in increasing property, and in adding to our national resources."

True enough; something or other has been made very

useful indeed in procuring food, increasing property, and adding to the national resources,—but not by any means physiology in particular: Might we not say as much of agriculture, of mechanics, of each of the arts, and of civilization in general?

Equally inappropriate is the next instance (page 14):—

“The upper line of diagrams before you points out the mode of development of one of the higher mammals—the dog. If we were to compare the matured ovum at one end of the line with the nearly completed foetus at the other, the mind might well be puzzled to determine by what steps the one was transformed into the other. But by tracing the successive changes which take place, we observe, how by a series of involutions and evolutions of a membrane, called the germinal membrane, how by its splitting up into three layers, each becoming thickened, and how, by then undergoing a number of turnings in and bendings out, the perfect creature is formed. Now, we do not say of such a development, the whole of which is hid from us, that the different stages constitute different animals; but in those cases where the progress is not hid, strange to say, this is what has resulted.”

He might have added—what would have been at least a shade nearer his presumed text—that the human foetus undergoes similar metamorphoses; embryotic man being, as Meekel says, distinguished from the other animals by the greater rapidity with which he passes through the inferior formations; but unless it were to show that he was well versed in embryology in its bearings on physiology, what could the lecturer possibly mean by introducing such facts in such a connection? How do they shew “what a study of the theory of medicine has accomplished for the improvement of its practice?”

He next alludes to the researches of comparative anatomists into the habits of certain parasites that infest the viscera of various animals, asserting that the modern treatment of tape-worm in the human subject constitutes one of the triumphs of theoretical medicine (page 16):—

“Man, like other animals, is apt to be infested by a peculiar kind of tape-worm called *Taenia Solium*, and there can now be no doubt, that this form of entozoon is a further stage of development of the *Cysticercus Cellulosæ*,* so common in oxen, sheep, and especially in pigs. These animals are largely consumed by man, and where their flesh is eaten raw,

* Dr Bennett seems to have forgotten—otherwise he would naturally have mentioned it here—that the *Cysticercus Cellulosæ* is itself found also in man.

as in Abyssinia, tape-worm is very common. In civilized countries, cookery destroys the vitality of the parasites before they are eaten; but if meat be underdone, or eaten out of season, a few may escape the action of cooking and of mastification, and so reach the stomach uninjured. That dogs fed on rabbits, or mutton flesh containing *Cellulosæ*, become affected with tape-worms, has been proved by the direct experiments of Kuehenmeister.

"But, you may ask, how do all such facts, interesting though they be scientifically, teach us to cure disease? At first, indeed, this is not apparent, any more than was the use of the latent theory of heat as developed by Black, or the utility of exciting spasms in a frog's limbs, by bringing them in contact with two metals, as was done by Galvani. But the theory of Black led to the construction of the steam-engine, and the barren observations of Galvani have enabled us to unite France and England by a telegraphic wire through the ocean. And, it may be assumed without the fear of contradiction, that it is such facts as I have alluded to, that are silently revolutionizing the study of medicine. Thus, if we want to cure tape-worm, it must be clear that it is not enough to give anthelmintics or purgatives; we must also prevent the eating of flesh underdone, or game or fish out of season, when it is likely to be infested with *Cysticerci*."

This is the third "striking instance," and far-fetched as we shall shew it to be, it yet has *some* presumed connection with the subject; it makes some effort—however futile when examined—at showing "what a study of the theory of medicine has accomplished."

In the first paragraph of the above quotation, some facts are merely stated: in the second, we have an example of false analogy and inconsequent reasoning—a kind of fallacies already more than once encountered in the lecture under review—in the present instance reminding us, we confess, of the fabled mountain in labour which brought forth a mouse. Steam and electricity, the two greatest wonder-workers of the age, are as it were too good to be lost as illustrations: no matter how, they must be got in. They are enlisted in the service of theoretic medicine accordingly, and lo! the tape-worm is evacuated by steam, and so effectually electrified that the patient is troubled by the astonished parasite thenceforth nevermore!

While we allow that the treatment of tape-worm which, besides giving anthelmintics, prohibits the eating of half-cooked animal food, reflects credit on those whose observations led to it, we entirely dissent from the assertion that

"such facts are revolutionizing the study of medicine;" and we repudiate the analogy of the steam-engine and the telegraphic wire. The observations alluded to (of Kuehnenmeister and others) were made on mice, cats, pigs, dogs, &c., for the detection of certain species of *cysticerci*, tracing them from lower to higher conditions and habitats up to man. On the other hand, the experiments of Blaek, Galvani, and their followers—which ultimately led to the construction of steam-engines and electric telegraphs—were made with elements and forces totally different—the imponderable forces heat and electricity. The things compared, therefore, are not merely different in kind, but the results as disproportionate in degree.

Loeke, in the early part of his career, was a physician, and we may be sure that—however ignorant of *cysticerci* and other parasites—when he reasoned as a physician he did so logically. Suppose Dr Loeke were to revisit this sublunary scene, and when viewing for the first time the action of a steam-engine or the working of an electric telegraph, suppose he were to be told, by some modern medical theorist, that the discovery of the habits and transmigrations of some entozoa, and some little improvement in the treatment of tape-worm since his day, are equal in importance, as scientific discoveries, to the results of steam and electricity—we may imagine how the resuscitated philosopher would stare. 'Sir,' he might possibly reply, 'as for the comparison you institute 'between things so different in kind and degree, I do not 'pretend to understand you, unless indeed my suspicion be 'correct, that however physic has advanced in theory, some 'of its professors have not improved as to their reasoning 'powers, or their common sense, since I practised as a phy- 'sician at Oxford nearly two hundred years ago. As for 'the tape-worm, we were familiar with that parasite, and 'the means of expelling it from the human intestines in my 'day. By administering a dose or two of the bark of the 'pomegranate-root (a remedy at least as old as the time of 'Celsus) we cured our patients, sometimes permanently, 'sometimes not; and the tendency of the complaint to recur

‘ sometimes puzzled us. But, at least, I can bear testimony to ‘ the truth of what you say about the importance of advising ‘ subsequent attention to the diet of such patients ; for it was ‘ matter of observation and experience, so long ago, that the ‘ parasite was least likely to trouble again those who ab- ‘ stained from fish, game, or other animal food, imperfectly ‘ cooked or out of season.’

We have next an instance of a vegetable parasite, in the skin-disease popularly known as “ scald-head,” medically termed *Tinea Favosa*. The microscope having demonstrated that the crusts (or scabs) which cover the scalp in this affection structurally resemble certain of the fungi, an appropriate theoretical treatment follows as a matter of course, which if neither very satisfactory nor original, as we shall show, is at least a vast improvement on the process of the *calotte*. Many barbarous practices prevailed formerly, and it would seem that scalping was not so exclusively confined to the Red Indians as our school-boy notions had given us to understand. We are told, page 17, that—

“ Formerly this disease was attempted to be cured by plucking out the bulbs of the hair with a pair of pincers, or by the barbarous practice of the *calotte*. This consisted of spreading a thick cohesive plaster over the shaved head, and when the hair had firmly grown into it, dragging it off forcibly, and so endeavouring to eradicate the disease and hair together. . . . But the discovery of the vegetable nature of this disease rendered it evident, that only those means hostile to vegetable growth would answer the purpose. For several years I have accomplished this, by simply shaving the head, and keeping the scalp smeared with oil, which, by preventing the access of atmospheric air, at once destroys the conditions necessary for the development of Fungi.

The *calotte*, we suspect, went out with the last of the barber-surgeons ; but there was another class of practitioners who professed to cure scald-head by milder applications, rivalling the famous oil of modern theorectics. Old women existed formerly—they exist yet—with faith in soap and water, especially soap ; and some of them have still the audacity to affirm that they can cure “ scald head,” and eke the itch, by means of persevering lather. And why not ? The barber-surgeons, the scalpers, the *calotte* practitioners, doubtless sneered at the old women of their day ; but the old wo-

men were right, and Dr Bennett has proved it. Perhaps, indeed, he has proved too much, for soaps, as we all know, are just oils with potash and soda added, and these alkalies having been found useful, and being medically accredited as separate applications in some skin affections, it may plausibly be argued that the lather of the old women is theoretically (ay, even theoretically) preferable to the pure oil of Dr Bennett.* Sooth to say, neither is a remedy much to boast of in *Tinea Favosa*—an affection mostly confined to childhood and youth, and for the cure of which time often does more than any other remedial measure; though wicked experience has been heard to prate of the frequent good effects of such applications as the iodide of sulphur ointment, but of course on no satisfactory theory whatever.

In connection with this subject of skin affections, a word about seabies (vulgarly, the itch), to which the lecturer alludes in a similar strain at page 22.

Most people know that soap and water suffice, in many cases, to cure seabies; and every medical man is aware that the disease depends on the presence of an insect, the *acarus scabiei*: “But,” says Dr Bennett, “to discover these insects, and to determine their habits, patient and long-continued research was necessary, by means of the microscope, and practice now reaps the benefit of it.”

If Dr Bennett means his kind of practice—the pure oleaginous treatment of seabies—then we say, decidedly, *No*, practice does *not* reap any benefit, but rather gains a loss; for those cases which do not yield to scrupulous cleanliness are certainly not curable by oil, and the itchy patient who lubricates his hide does but lose time and labour. As there

* We are far from anxious to underrate the therapeutic virtues of oil, especially the cod-liver oil, when internally administered in incipient phthisis, and the strumous diathesis generally. Neither Dr Williams of London, however, nor Dr Bennett of Edinburgh, both of whom claim the merit of its re-introduction in practice, would deserve much credit, even could he make that claim good, compared with the obscure country practitioners, and sensible old women of our own sea-coast villages, and those of the Continent, with whom it found acceptance and a refuge when it was slighted or forgotten by all the Colleges of Physicians.

is no denying the fact that the oleaginous treatment often fails, recourse is had to a theory to explain the failure, as if the failure were not sufficient, or the object was to make fact obedient to sacred oil—and we are told, that—

“In chronic cases the eggs, however, remain, and hence other applications may sometimes be necessary,” &c.

Now, one would suppose that, if oil can asphyxiate the *acarus* when it comes out of the egg, no other application should be necessary to cure any form or stage of scabies; unless, indeed, the said eggs are a vast deal longer in hatching than startling facts lead us to believe. The fact remains, however, in spite of “the patient research by means of the microscope” of which we hear so much, that in serious cases of scabies we are obliged to fall back upon an old wife’s remedy once more, and to accept, with as good a grace as possible, the nasty, empirical sulphur ointment!

So much for the kind of facts as to which Dr Bennett gravely says, “it may be assumed, without fear of contradiction, that they are revolutionizing the study of medicine.” (!)

There is something that would be ludicrous, if it were not also mournful, in the peep-show magniloquence with which these microscopic wonders—these liliputian attainments are trumpeted forth to a generation of mankind asking, not for such a kind of glad tidings, but for one grain of hope in the prospect of another visitation of Asiatic cholera, or, it may be, some other disease as new and fatal, before which when it suddenly comes theoretical medicine will, not for the first time—stand silent, or retreat apalled. Whence all this mere varnish for outward application? What avails all this pompous jargon about scab and itch, whilst we have not one fixed principle to guide us, not one ascertained remedy to arm us, in the treatment of those sudden and awful epidemics which we have seen before, and will assuredly see again, decimating a population calling to us in vain for help—reproaching us as it were with their last breath, haunting us with their dying looks? Away with your seabed-heads and itchy extremities! Science, you say, is revolutionizing

medieinc,—theory, you affirm, is confounding blind experience; give us, we pray you, serious proof of it. Give us some more evident, some less microscopical fruits of your glorious theory and your omnipotent science. Teach us how to vindicate their majesty, insulted by an incredulous, a quack-ridden, a pestilence-smitten public;—tell us how we are to answer the professional sceptic who points exultingly to Vienna homœopathic statistics, to French expectant treatment reports, in proof of the alleged fact, that when it comes to the push, when such diseases as typhus fever and Asiatic cholera become the test of power in practice, results confound us all. In a word, can your theory, or even your microscope that reveals so much about seb and itch, do anything more for us,—can it teach us to save life? This, we take it, is the prime question for us. Science—*genuine* science—is in no danger of being mocked; she will ultimately vindicate herself, but death will not wait. We ask you again, are you willing, are you anxious to be judged by your fruits—and if so, where are they? Alas! in this lecture of Dr Bennett's, the professed object of which is to set forth the triumphs—the practical triumphs over disease of theoretical medicine, we can find only such fruit as crumbles to dust when we touch it: as the weary traveller, already sick of mirage, standing on the blasted shores of the Dead Sea, is mocked by the painted apples of Sodom!

In strange contrast with this exaggeration of insignificant observations, this inflation of paltry results in practice, Dr Bennett subsequently alludes (page 19), with apparent indifference, to one of the few important observations involving theories of disease, which are really, in some sense, tending towards practical revolutions in medicine. He denies the truth of the observation made by Dr Alison, and other observers of eminent experience, that some inflammations, like fevers, have changed their type since the days of Cullen and Gregory; and having denied it, he dismisses the question as of no further consequence.

To be sure it does not admit of microscopical investigation; but supposing it likely to be true (and the evidence for its

truth is so remarkable that prejudice only can venture to ignore it), how infinitely does such an observation as this transeend improved treatment of tape-worm, oleaginous application to scalp of dirty boy, and eke theoretical smothering of aceri! Seems it not amazing—where so much previous ingenuity had been exercised in ferreting out the microscope ills exceptional skin is heir to—that this vital and comprchensive speculation (supposing it no more in the meantime) should be so lightly jumped over? If found to be correct, what an important fact, in the history of disease on the large scale, is this on which Dr Bennett turns his back! Of what consequence to mankind that physicians should determine, one way or other, the alleged fact. Dr Alison may possibly be right. Why not? If it be true—and it *is* true—that disease has undergone vast historical revolutions, why may not special diseases change their type? —it is at least natural and likely that they should.

It is matter of undoubted fact, in the history not so much of medicine alone as of the human race, that great diseases have undergone great revolutions; that certain of them have disappeared, and that new ones have appeared from time to time amongst mankind. Where now is the true leprosy, once the most loathsome and the most dreaded pestilence of the civilized world? It is confined to Eastern India and the extreme north of Europe, where even it is on the decrease. Syphilis, which appeared as the successor of the true leprosy in Europe, and whieh is therefore not a very old disease, seems in its turn destined to give way ere long to a new disease, perhaps generically allied to it, and, may be, even shorter lived. As a European disease, syphilis seems to be on the wane; in its most malignant form it is now comparatively rare, and in no form can it be said to rage as a pestilence. On the other hand, the Asiatic cholera is quite a new disease; so new, and so little understood that, for all purposes of cure —nay, of palliation even—the most accomplished and experienced physician who finds himself face to face with it, seems powerless as its victim.

Yes, it is at such a test as this—in the presence of a new

and terrible disease—*infections*, coming suddenly and sweeping off multitudes of mankind ruthlessly, yet as it were *egregiously*, that Theoretical Medicine, with all her pretended scientific attainments, stands dumb, confounded, and ashamed, hiding her microscope in its proper corner! By and by, the pestilence having overpassed, she re-appears microscope in hand, and tells us, perchance that (page 22) :—

“ All these contradictions depend on imperfect attempts at correct theory ; and this latter once rendered perfect, it will be seen that both health and disease are governed by laws as determinate as the motion of the planets, and the currents of the ocean.

So says discomfited Theory, putting on the greasy mask of propheey ; and were like things compared with like, perchance we might be induced to accept so flattering a prediction. The laws that govern the planetary motions and the tides were once unknown ; but, even before their discovery, it was at least certain that they were regular and uniform in their operation. Unfortunately, the laws that govern disease—whatever, or however discoverable they may be—are very evidently the reverse : indefinite, changeful, fugitive, as disease itself. They are—they must be, speaking of them as unknown—subject to what disease is subject to, atmospheric, climatal, periodical, individual influences ; a number of irregular and unpredictable emanations. Yet the laws that govern disease—intangible though they be as the air on which they ride—*may* yet be demonstrated, explained, brought within the narrow enclosures of medical *surveillance*. The millennium has yet to come. But, at present, it is a little too much to assume that the unknown laws supposed to govern disease are *like* the known laws that govern the planetary motions and the tides : it is irrational to compare the discovered and the immutable, with the undiscovered and the mutable.

Having, in the face of historical evidence and living experience, denied that fevers and inflammations have ever changed their type, Dr Bennett, with seeming consistency (it is only seeming), ascribes the diminished mortality in pneumonia in recent times exclusively to the comparative

abandonment of blood-letting in the treatment of that affection. But, we respectfully beg to ask him, *What led to this abandonment of blood-letting in pneumonia?* And here we fix him betwixt the horns of a dilemma. For either it was blind experience, or a theoretical demonstration of blood-lettings' *modus operandi* in pneumonia, that led to the change of treatment. If he should say blind experience, then would he admit what is contrary to his principles—that blind experience is a good and safe guide in medicine, since in this important instance it has led to diminished mortality; if a demonstration of blood-lettings' *modus operandi*, then where is that demonstration? or if it exist what is it worth? But such a demonstration exists not, or if it exist is worth nothing; because it has not yet been determined even what set of capillaries is specially engaged in the inflammation of pneumonia.* Therefore, either it was blind experience that reduced the mortality in pneumonia, or else

* Dr Williams, one of the very few writers who have expressly alluded to this question, in his article on pneumonia, in the 'Cyclopaedia of Practical Medicine,' observes:—"These examinations and some pathological considerations, induce us to consider the capillary ramifications of the pulmonary artery and veins to be the proper seat of pneumonia, and that these may involve more or less of the tissues through which they pass." This, we presume, is also Dr Bennett's opinion. A very different, and, we suspect, a juster view, is advocated by Dr Morehead, in a clinical report of 103 cases of pneumonia, treated by him in the Fejeebhoy hospital at Bombay—a most valuable series of observations on the disease, extending over a period of six years, rerecorded in the 'Transactions of the Medical and Physical Society of Bombay' for 1854. He says:—"The capillaries of the bronchial arteries are the nutrient vessels of the visceral pleura, of the mucous lining, and other structures of the bronchial tubes, and of the connecting areolar tissue of the constituent parts of the lungs. We cannot avoid the conclusion, that they must also be the capillaries concerned, when the inflammation is of the pulmonary cell walls, and of the areolar tissue that connects the cells to each other.

"The capillaries of the pulmonary artery, on the other hand, convey venous blood to the air cells, to be distributed on their walls, in order that the physical process of endosmosis and exosmosis may take place between the gases of the blood and of the atmospheric air. It cannot be looked upon as probable, that the blood in these capillaries takes any part in the vital processes of nutrition of the cell walls. It is, therefore, a just conclusion, that these capillaries and their blood cannot be agents in the altered state of nutrition of the pulmonary cell walls, and their connecting areolar tissue, which we designate by the term pneumonia."

(there is no alternative) the disease has changed its type since the days of Cullen and Gregory. Dr Bennett may take his choice.

Let us now turn to the latest statement of a fundamental doctrine in medicine, and see what profit theory derives from the, so-called, cell pathology (Lecture, page 17):—

“ Although fifteen years have elapsed since the cell doctrine of growth has been admitted into physiology and pathology, medical men have not yet realised to themselves its vast importance in a practical point of view. Professor Virchow of Wurzburg, indeed, has recently endeavoured to replace the doctrines of solidism, fluidism, and vitalism, by that of what he calls the cell pathology. But I have taught the cell pathology for the last fourteen years in this school, and have gone further, by showing that it is no more universally applicable to the phenomena of disease than is humoralism or solidism. Indeed, we may more correctly speak of a molecular pathology, as a molecule, and not a cell, is the first and last form of organisation. But molecules, in their turn, are deposited from fluids, and so we again arrive at a species of humoralism.”

Is it possible? Is it indeed true, that, after so much dis-course has been wasted, after so many wordy volumes (upon cell growth) have been given without enlightenment to a gaping professional world, we are not ashamed to retrograde a century or two, and e'en exhume the mouldering humoral pathology of our great-grandfathers! *And thou too Brutus!* we instinctively exclaim, after having read Dr Bennett's victorious peroration, wherein he says,—

“ Everywhere we see Natural Philosophy advancing—enthusiastic chemists pushing forward organic analyses—anatomists, unwearied in their researches concerning development and the structure of tissues—physiologists experimenting and concentrating all the resources of modern science, in order to elucidate organic laws—and pathologists busy in connecting the symptoms observed in the living with alterations in the minutest tissues and atoms of the dead. At this time medicine is undergoing a great revolution,” &c.

Vox, we say, vox et præterea nihil. A great revolution indeed! not unlike some other revolutions witnessed in these times, resulting in zero—that is to say, in fluid—fluid frozen for a century! Yes, after ages of research and a sinful waste of time and study, it appears that we are returning to an exploded humoral pathology.

A startling fact this, suggestive of some surmises, per-

haps more philosophical than pleasant or hopeful, for the curious disciple of *Æsculapius* to indulge withal. Does the history of medicine, like the history of the philosophy of mind, really describe nothing but circles?—looked at nakedly, stripped of the robes of theory, is medicine at length discovered to be the eternal serpent with its tail in its mouth? Few who have curiously perused some of the older medical writers (*e. g.*, *Paulus Ægineta*) have missed the conviction that, beneath a nicer phraseology and buttoned in a tighter dress, we vain moderns are playing not so much original tragedies or farces as mere revivals in medicine;—revivals, not alone of old plans of treatment in disease, but of ideas and theories also. So that, often, when modern medical writers profess to deal with principles or with theories, we can readily find something nearly amounting to a parallel in the ancient authors. In the last page but one of his lecture, Dr Bennett talks much as a more famous physician did some two thousand years ago, when he says—

“This conviction (to wit, that health and disease are governed by laws of an exact and determinable kind), is now everywhere gaining ground, and the public are beginning to distrust the man who merely boasts of his experience and the action of his drugs, and to place confidence in him who treats according to natural laws, and simplifies his remedies. Even quackery has changed its features,” &c.

Asclepoides was much of the same opinion in his day. He also affected to abhor and despise what he vaguely termed quackery (doubtless meaning thereby all the medical cries of the day with the exception of his own gallinaceous cackle), and to undervalue previous experience. He also affected to treat disease “according to natural law” (yet to be discovered), and to “simplify his remedies.” He also reduced the phenomena of disease to a doctrine of atoms and pores, very like, one would say, our latest molecular pathology. He also opposed blood-letting in inflammations (and, of course, in pneumonia); he advocated a tonic regimen in disease generally; recommending gestation, the use of wine, and the external and internal application of cold water, particularly the use of the shower-bath, of which he seems to

have been the designer: he made much of the division of diseases into acute and chronic, and had faith in frictions (with oil?) True, the ancient, unlike the modern Asclepiades, had neither microscope nor stethoscope; but it by no means follows that he was unblessed with an acute pair of natural eyes, nor is it the less likely that he was in the habit of applying his ear to the chests of his patients.

But let us now see what kind of practice our modern Asclepiades would have us found on a retrograde cell pathology (page 19):—

“The growth of tumours may be encouraged or retarded by the same means which influence all kinds of cell development. But, if they assume a parasitic character, as in cancerous growths—that is, if the cells possess a power of multiplication in themselves—then the only chance of cure is in their complete destruction or extirpation. But the surgeon who trusts to his naked sight, forgets that germs are infiltrated among the surrounding tissues. These he cannot see from their minuteness, yet he employs no microscope to discover them. Need we wonder therefore, that they should frequently return, or rather grow again, as in fact they have never been thoroughly removed.”

Here, limiting himself to a glorification of the microscope at the expense of operative surgery, Dr Bennett overlooks a fact with which, of course, he is familiar, to wit, that cancer is a disease of the blood. This fact, though a proof of his wisdom in embracing the ancient humoral pathology, demonstrates the absurdity of the practice he would have surgeons adopt; for if cancer be a constitutional, and not mainly a local affection, it is plain that the microscope can be of little use in the treatment of it. Constitutional remedies, directed to the alteration of the constituents of the blood, are surely the only medical means calculated to ameliorate or cure the cancerous diathesis. As for the external manifestation—as for the cancerous growth or tumour—experience having proved that, under certain tolerably well defined circumstances, this may be removed with much immediate relief, and the prospect of permanent immunity to the patient—the surgeon removes it accordingly. As a guide to the performance, or non-performance of such an operation, he has something much better than a microscope to consult; he has, that horror of your affectedly scientific physician, a

symptom. The surgeon (sadly practical man!) examines his patient's glandular system, and extirpates, or not, the tumour, smiling perchance the while—as well he may—at the harlequin idea of thrusting a microscope into the wound in search of germs which may be “infiltrated among the surrounding tissues.” Regarding the tumour itself as a symptom, he treats, he extirpates that symptom, and thus vindicates the truth of a practical principle which theoretical medicine nowadays affects to contemn ; to wit, that it is often necessary, generally wise, to treat diseases in direct relation to their symptoms.

And this brings us to notice one of the lecturer's general observations, wherein he mourns the ignorance and incapacity of his professional brethren in regard to the use of the stethoscope (page 19) :—

“ Notwithstanding the universality with which the stethoscope and auscultation are now received as necessary means of diagnosis, how few of our medical men, comparatively, are really skilful in detecting by them the morbid changes going on in the heart and lungs.”

Now, despite the above modest and affecting reflection, there really never was a time when the detection of diseases of the heart and lungs, so far as can be accurately accomplished by means of the physical signs alone, belonged more to the profession at large, and was less justly the boast of any one coterie or any one man. It is peculiarly in this kind of acquisition that the hospital clerk and the advanced student may be said to gall the heels of the oldest physician : and why should it not be so ? What mystery is there, or what cumulative knowledge demanding long study and research, in the tapping and listening we call percussion and auscultation ? The truth is, these processes require little or no mental effort, and are peculiarly independent of general professional attainments ; so much so, that the relative position and healthy sounds of the heart and lungs once acquired (and it is a mere mechanical lesson), a clever mechanic, without knowing anything more of medicine, might become an expert auscultator. To be sure, he might not readily attain that faculty of minute diagnosis of which your

stethoscopic enthusiasts are for ever boasting ; but besides that great minuteness in physical diagnosis is generally useless for purposes of actual treatment, there are good grounds for suspecting that it is often either an imaginary gift, or fatal, in some incomprehensible way, to the *ordinary* observing powers of those who possess it. If the glaring mistakes, as well as the alleged refinements in physical diagnosis, of the loudest talkers, were allowed to transpire, the public would be astounded to learn how brittle a weapon the stethoscope sometimes is, in the hands of a dogmatic physician.

In proof of this, we shall quote a passage from a work written, it is true, in defence of a system of which we are no disciple, but whose author is unquestionably a man of great ability, and second to none (whatever may be thought of his modes of treatment) in the detection of disease. We consider Professor Henderson's testimony on this subject as unimpeachable as it is significant: in his "*Homœopathy Fairly Represented*," he says :—

"I have *known* an accomplished consulting physician, and an eminent general practitioner, overlook or mistake double pneumonia of great extent, and discover it only on dissection ; I have *known* a great advocate of cod-liver oil in consumption, mistake chronic pleurisy for the other disease ; I have *known* an eminent stethoscopist, for mere irritation of the throat, which he treated with caustic as usual, mistake pulmonary consumption, which was fatal within the week, by the bursting of a tubular abscess into the pleura. I have *known* an instance in which a notable hospital physician, not finding on dissection, the pulmonary disease he had mapped out and described to his pupils, adroitly remarked, 'Gentlemen, you perceive the appearances on dissection don't correspond with the stethoscopic signs heard during life ;' (the lung was sound)."

The above revelations will perhaps enable us to view in a different light from that in which Dr Bennett has set it, the ensuing passage of his lecture (page 20) :—

"I cannot too strongly caution you not to be influenced by the opinion of those, who, educated before these means of research came into general use, speak of them as worthless, especially in the investigation and diagnosis of disease—because, in short, they are ignorant of their value, therefore, forsooth, they can be of little benefit. I need scarcely remark that this kind of reasoning is altogether unsound, and is directly opposed to the introduction of all improvement in either science or art. What should we think of a modern astronomer, who boasted that it was enough for him to examine the heavens with his naked eye, and sneered

at telescopes? or how should we like to trust ourselves at sea to the navigator, who, as in ancient times, steered his course by the sun and stars only, and who abused sextants and other instruments by which alone exact results are arrived at? Such, however, is exactly the position of those medical men who consider stethoscopes and microscopes useless, and thus betray an unacquaintance with the present state of their own art."

Unfortunately, at least for Dr Bennett, the comparison here instituted between certain medical men who consider stethoscopes and microscopes utterly useless, and certain modern astronomers and ancient mariners who sneered at telescopes and quadrants, is a pure romance. There is no question among medical men, old or young now, as to the *uses* of stethoscopes and microscopes; it is only in the minds of the public that prejudices regarding these much talked of instruments can be truly said to exist. Every medical man knows that a stethoscope is nothing else than a piece of wood, adopted out of delicacy or cleanliness; not to assist the ear, but to keep the head and face of the auscultator at a respectful distance from the chest of his patient. It is, however, a popular idea—which those who are really zealous for the suppression of quaekery should lose no time in correcting—that, like the telescope, the stethoscope also is a scientific instrument, capable of bringing into notice what the naked sense is too limited to discern.

As to the microscope, its proper province is that of minute anatomy, sound and morbid, not the practice of medicine. Its occasional application to the purposes of the physician,—to the examination of parts of the human body after death, or those products of the living body which have ceased to be vital at the time of the observation (*e. g.*, the blood and urine), is universally admitted. On the other hand, it is allowed that microscopical research can, of itself, no more make a man an accomplished physician, than can the study of anatomical drawings make him a good surgeon; the former may, however, if too much harped on, have a tendency to narrow the mind, obscure the general intelligence, and rather unfit its devotee for the enlarged practice of his profession. There are certainly eminent men who think, and who have the courage to say what is not fashionable at present, but

not on that account the less true—that we run great danger by placing dependence on such appliances (for the stethoscope and microscope are no more than appliances), at the expense of other means of diagnosis, much more valuable in kind and degree.

It has been the mode of late to underrate the importance of *symptoms* in disease, because they are matters of simple observation and experience; as if there were any other constant guides, or general landmarks in practice. The stethoscope—that is, the ear—may often, it is true, remarkably assist in the diagnosis of a chest affection (although, as we have seen, it is very far from being an infallible aid), and the microscope throw light on a renal disorder; but symptoms can convey far more and reach much further, for, besides being common to all diseases, whether of the viscera, the blood, or the nervous system, they often indicate the appropriate treatment, or at least lead to palliation where everything else is doubtful. How often does it happen that nothing else can be done,—that the physician, having nothing else to guide him, is compelled to treat the symptoms—and the symptoms of disease alone!

The most dangerous, because in some respects the most plausible, fallacy of modern medicine, is that which proposes to combat disease, in general on scientific principles; such principles being, on the whole, quite inapplicable to an art of healing; an art which must always depend mainly on observation and experience, and in proportion as it loses sight of these at any time, must lose ground, by forgetting or neglecting the lessons of the past, again necessary to be relearned. Thus it is that medicine moves, not in straight lines but ever in circles; thus it is that the otherwise inexplicable fact becomes natural and consistent,—the fact that, to-day—as it will doubtless always be more or less the case—we are reviving the remedies and the theories that fretted their brief hour, on the same stage, long centuries ago.

That science shall much avail to conquer disease, is the dream of the enthusiast, or the specious tale of the march-of-progress charlatan: That medicine, with its admitted no

primitive fact, with its only possible definition of a *healing art*, should ever become an exact science, is as if one language should become common to all the tribes of the earth, and one political government unite the nations of mankind in a universal and settled harmony. That disease with its changeful cycles, its startling revolutions, should ever stoop to the government of fixed laws, is as if the winds of heaven should again be confined in the cave of *Æolus*!

When chemistry* becomes alchemy,—when metals are readily transmutable, and physicians cease to demand, because they no longer require, the fees of their patients,—then, but not till then, will science and medicine form a rational and lasting alliance.

We have now done with Dr Bennett's lecture,—a production, we make bold to say, remarkable for the narrowness of its views, and the absurdity of its illustration.

* Chemistry, we conceive, is destined to be the one strong point in the future attempts that will be made (alas, in vain!) to erect a scientific scaffolding for medicine. The decomposition of *animal* gases will be successfully attempted in large towns, as an important hindrance to the spread of fevers and choleraic diarrhoeas, because the attention of legislatures will at length be forcibly directed to the important subject of public hygiene; but chemistry can never be made available in neutralizing vegetable or subterranean emanations, on the large scale. In tropical and semi-tropical climes, slimy rivers with mangrove banks, luxurious forests, and the rank vegetation of alluvial savannas and lovely but treacherous oases, will, for long centuries to come, inevitably give rise to miasma, more or less fatal to human life wherever it may be wafted, on the wings of the wind, over this terraqueous globe. And when we consider that vast regions of Central America are yet but partially explored, while the whole of the enormous Continent of Africa, with the exception of a narrow inhabited coast-belt, is still a *terra incognita*, probably consisting of alternate forests, mountains, swamps, and deserts—it becomes at once apparent that chemistry can be of no avail in preventing, or even checking, those diseases we term pestilences, infectious diseases, and epidemics. Moreover, the gases which issue from volcanic openings, craters and fissures in the earth's surface, probably are, and will continue to be a subtle source of mysterious diseases.

APPENDIX.

Although the first impression of this pamphlet was out of print in a few days after its appearance, there was at first no intention of re-issuing it; but it has excited so much more attention than was anticipated, and has been so condescendingly noticed by some of Dr Bennett's friends, that we feel bound to supply a second edition, since they will have it so: Taking the opportunity of observing, for the information of those who have since spoken or written in Dr Bennett's defence, that our pamphlet was (and is) simply a review of his lecture, similar in kind to the notice taken regularly (in the *Reviews*) of other the like published papers on scientific or literary themes; and not by any means, as some affect to consider it, a personal attack on Dr Bennett.

If those whom we have offended mean, that we were bound in courtesy to approach a criticism of Dr Bennett's lecture "with bated breath and whispering humbleness," in consideration of the mild and amiable manner in which that gentleman himself has been in the habit of criticising others,—then we partly understand them: the only pity is they have not spoken plainer. Possibly Dr Bennett has been very careful of the feelings of his professional brethren, and remarkable for his own want of assumption; perhaps he has been notoriously averse to criticism, and modest, overmuch, in comparing the performances of others with his own. But what then?—it is with the author of the published lecture—"The Present State of the Theory and Practice of Medicine"—not with Dr Bennett as an individual, that the writer of this pamphlet has presumed to deal.

We shall now turn a contemplative glance on a defence of Dr Bennett's lecture* lately published, not by himself, but as we understand, at his desire and under his own supervision. Had he employed a strictly professional advocate,—still more had he deigned to strike openly for himself, we should perhaps have devoted a separate pamphlet in reply; but his redoubtable *M.A.* (for he is not a member of the medical profes-

* "Reply to Dr M'Gilchrist's 'Remarks' on Professor Bennett's Introductory Lecture. By John Glen, M.A. Edinburgh: Sutherland & Knox.

sion) having put himself in the position of an Appendix to Dr Bennett, he must be content to figure in that situation to us also.

Mr Glen (a gentleman known to the literary world as the author of a somnolent prize essay of which, we believe, Dr Bennett was one of the adjudicators) begins by contrasting Dr Bennett's lecture with these "Remarks":—

"In regard to these two pamphlets, I may first express my general opinion. The lecture of Dr Bennett appears singularly pleasing, from its hearty and hopeful tone, from the prominence of distinct ideas, the clear method by which each idea is in due succession developed, and the elegance of the illustrations no less correct in themselves than adapted to the audience before whom they were delivered."

As the individual opinion of the commentator, no fault can reasonably be found with this encomium: to him the lecture may certainly have been as "pleasing" as he afterwards says our critique of it is "disagreeable," and as far as he was concerned, it may have appeared well "adapted to its audience." But a very different estimate of it has been formed by a very different reviewer. The medical profession have been told by the *Medical Times* of February 9th last—"that if taken (as it is intended to be) by the student as his guide in the prosecution of his professional studies, it will inevitably lead to disappointment." It is not then quite so certain that it was *well adapted to its audience*, unless that audience was a packed one. Neither is it quite certain that Mr Glen does not betray a little amiable partiality in a subsequent whitewash which he applies:—

"It is unnecessary further to express our high opinion of Dr Bennett's Introductory Lecture. It is admirable for the sagacity with which the leading question of the day is singled out as a suitable subject for an opening address—for its enlarged views of the relations of medical science—for the elegance and interest of its illustrations—for its methodical arrangement and sound reasoning."

Hear the other reviewer:—"Successfully to perform the task which Dr Bennett has assigned to himself in this lecture, would demand higher qualifications than he has brought to bear upon it. It would require at least a competent knowledge of philosophical subjects, and a capability of philosophical discussion;—a deeper acquaintance with science than can be obtained from superficial manuals, and a larger experience than even hospital practice can bestow."

Now comes our turn—on the wheel:—

"In the criticism of Dr M'Gilchrist will be found a painful contrast. It is disagreeable to the unbiassed from the unhealthiness of its tone, which betrays captious irritability against an individual professor, groundless despondency in regard to the advance of medical science, and undue depreciation of the advantages of Theory. It is disagreeable to the logician from the absence of prominent points and orderly arrangement; from the frequency with which premises are assumed, which Dr

Bennett might reasonably dispute, and the manifest inconsequence between premises thus or otherwise assumed, and the conclusion deduced. To one who has carefully perused Dr Bennett's lecture it is disagreeable, from its misrepresentation of his views, incompetence to understand his arguments, and interpolation of fond theories."

We need offer no apology for following Mr Glen's example, and expressing our "general opinion" of his pamphlet. It is a piece of laborious pedantry, aiming at something not very difficult to discover (retaliatory), but pretending to more. It is the hard, hard work of an obliged advocate, with a shocking bad brief: it is "full of sound and fury," but it *signifies* very little. It reminds one of a scene in the *Vicar of Wakefield*: here it is—

"' Big with these reflections,' says Goldsmith's hypothetical logician, 'I sat down, and finding that the best things were to be said on the wrong side, I resolved to write a book (pamphlet) that should be wholly new. I therefore dressed up three paradoxes with some ingenuity. They were false, indeed, but they were new. The jewels of truth have been so often imported by others, that nothing was left for me to import but some splendid things that at a distance looked every bit as well.'

* * * *

"' Well said, my boy,' cried I; 'I hope yon did not pass over the importance of the monogamy. But I interrupt; go on. You published your paradoxes; well, and what did the learned world say to your paradoxes?'—'Sir,' replied my son, 'the learned world said nothing to my paradoxes; nothing at all, sir.'"

Whatever the learned world may say to Mr Glen's paradoxes, we mean to pass by most of them, to which as they stand, 'pure and simple,' Dr Bennett is most heartily welcome; and much consolation may he derive from them! We restore him, inviolate, Mr Glen's terms and premises, both major and minor, with their resulting progeny of syllogisms.—(By the way, surely this *M.A.* is syllogism-mad: at his time of life most men have got rid of the schoolboy conceit, that it is smart to ring stale changes on abstract logic.) We decline entering on the "Theorumata,"—the "Theory of Parallels," "General Theory of Equations," "The Theory of Indices," "Solar Theory," "Lunar Theory" (especially), "Elliptic Theory" (and we shan't "vid. Herschel, p. 307, last edition"—certainly not), and "the Theory of Universal Gravitation"! No; we can do without them, as Mr Glen will peradventure find before he has digested this little Appendix. Also, we had rather not consider, however shortly, "the influence of science A on art A, of science B on art B, of science C on art C," &c., &c.

We have, however, some notice to take of those parts of Mr Glen's pamphlet, which are most pertinent to the medical points in criticism, beginning about the middle of it:—

"Let us, then, faithfully consider Dr Bennett's list *seriatim*, and then estimate the conclusion which he propounds.

The first instance is the application of science to the treatment of those afflicted by the *taenia solium*. The special department of medical science which is, in this treatment, applied, is human and comparative embryology ; and the special truth in embryology which is directly applied is the curious fact, that the *taenia* and the *cystoecercus* are two phases of one living creature. Dr Bennett does not wrench this fact out of its historical connection, as if it were an isolated, accidental, blind observation. He shows (and in a most interesting manner) by a series (as it were) of tableaux the successive stages in the development of the truth, and the gradual preparation of men, by one stage, to suspect, seek for, and comprehend the subsequent ones. The first stage is the preservation of the cloud of oyster ova in the water of Lake Fusaro, with a practical application for the increase of food. The second is the discovery that the ova of fish may be artificially impregnated and nursed, with a practical application again for the increase of food. Third, The curious investigations into the canine embryo, with this result only, that scientific men become deeply impressed with the conviction that different forms do not argue a difference in the living individual. Fourth, Investigating under this strong conviction, some scientific men discover that four so-called kinds of animals are but different forms in the history of one kind. Fifth, Then arises a suspicion that the *cystoecereus* and the *taenia* are but two forms of one entozoon. Sixth, The suspicion is pushed on to proof by Knechenmeister. Seventh and last stage in this eventful history is the practical application to disease."

This is a most laboured attempt to reconcile a theory, as it were, of a writer's meaning, with the order and spirit of his words ; and it is as remarkable for its assurance, as for its ingenuity. It is asking one to read Dr Bennett's lecture backwards, sideways, obliquely,—any way but straightforwards. The tape-worm may be the third or fourth (we are not quite sure which Dr Bennett meant it to be), but, beyond doubt, it is not the *first* of the "striking instances" advanced directly to prove what "the *theory* of medicine has recently accomplished for the improvement of its *practice*." Why, pray, did not the commentator place Dr Bennett's own words in juxtaposition with his explanation of their meaning ?—it would not certainly have furthered his main object—which, we think, was to *bamboozle*. He says—

"The greater portion of this elaborate instance is misapprehended by the critic : he believes that Dr Bennett is adducing the first steps in this argument to prove the application of scientific truth to medical practice, whereas he adduces them as but links in the advance of science from truth to truth onward to the final discovery of *taeniae* and their formation —the conditions favourable for their development, and the conditions sufficient for their destruction : hence Dr McGilchrist's criticism about futile arguments must fall to the ground."

Now, we unhesitatingly assert that, of 1000 sane readers of the lecture, not more than two or three of the most mystical-minded would solve this "elaborate" problem otherwise than we have done,—that is,

without Mr Glen's assistance: For, if his explanation of what Dr Bennett meant to convey, by the *order in which he himself set*, and we correctly quoted, his 'striking instances,' and of the chain of ratiocination he wished to adopt throughout, be the true one,—then is Dr Bennett not properly acquainted with the usages of the English language, but so ignorant of syntax that he is incapable of writing a lecture at all, or at least such a one as may convey, otherwise than by chance, the order and sense in which he wishes his language to be taken. But, as no one can believe this, we are shut up to the conclusion that his advocate has grossly misrepresented him.

In allusion, further, to the astonishment we expressed (at p. 12) at Dr Bennett's ingenuity in evading the tape-worm by steam and electricity, Mr Glen says:—

"The reply is ready. Dr Bennett makes no such direct comparison between uses and results: 1st, Because it is beside his purpose, which was to point out that practical inventions in our days may trace their origin to very unlikely truths; and 2d, Because he is too wise to estimate what is ever shifting in value. Thus, the electric telegraph is most important at this moment, for Government desires prompt information touching the state of the army. That information gained, the telegraph becomes of less importance and steam-engines rise in value, for troops require to be transported and telegraphs cannot do that. But neither telegraphs nor steam-engines will console a man who has tape-worm in his bowels [we should think not indeed!]. The important thing for him is to get rid of the worm, and, further, to be warned how to avoid getting any more of *their* breed. The criticism is, therefore, a misrepresentation; and yet it is to hear this misrepresentation against an Edinburgh medical professor, that John Loeke, of immortal memory, is raised from the dead. We are warned by common sense not to awaken a man abruptly from a sound sleep—he is apt to be out of temper. If the same rule prevails in Pluto's realms, the reason of *John M^GGilchrist Locke's* snappishness of manner is explained:—otherwise, his temper must have altered its type (like pneumonia), but for the worse, since he practised in Oxford 200 years ago."

This is a very pretty passage as it stands, and it speaks for itself. It needs no visitant from "Pluto's realms" to tell what dictated the playful back-hander administered in the last part of it: it was the erine of criticising an Edinburgh medical professor, and that Edinburgh medical professor Mr Glen's patron—one had been pardoned else for *raising* Loeke.

If we rightly understand him (for hereabouts, as frequently elsewhere, the *M.A.* waxeth somewhat uebnlous), it is also in reference to our comments on Dr Bennett's famous analogy between the electric telegraph and steam-engine, and the (alleged) improvement in the treatment of tape-worm, that Mr Glen accuses us of calling the analogy '*impossible*.' But we nowhere use the word impossible in this instance; and we doubt much if we shall ever again make use of it at all, after wading through

Mr Glen's *lucid* pamphlet. Surely nobody who had ever dipped into *Joe Miller* would have the audacity to affirm, that any analogy, however absurd, was impossible. But, certainly, if there be any one thing here more impossible than another it is, one would say, the astonishing feat Mr Glen has attempted, viz., to whitewash this very analogy, in regard to which the reviewer in the *Medical Times* says:—"What analogy is there between Black's discovery and the steam-engine, and the impregnation of the spawn of the frog and some undeclared and unknown improvement in practical medicine? Oh, shame that platitudes of this kind should be palmed off in such a University as that of Edinburgh, and from a chair in which the modest and philosophical Alison once sat!" The italics only of the last sentence are ours, and Dr Bennett has to thank Mr Glen for the severity with which we are reluctantly compelled to lay the lash on Again:—

"Dr Bennett's second instance," says Mr Glen, (but it is *not* his second instance,) "is—

"The cure of favus, as a vegetable mould, by constant exclusion of air from the spornles and thalli; and the vast improvement in point of gentle and successful treatment is, with great interest, unfolded. But the critic declares, that old women cure the same disease by very gentle practice, viz.—good soap and water. Now, query, is it really favus which these females treat—query, are they indeed successful in curing true favus—and, query, in what time are they successful? Dr M'Gilchrist evidently takes for granted that old women can diagnose true favus from eczeimas (in which belief he is very simple, and may stand alone). He does not state in what time, but he is sure they do cure it. On Dr Bennett's authority let the statement be denied."

We take the liberty of making Dr Bennett answer the first of the above queries for us: he it is who virtually admits that old women can diagnose true favus, for—"Here," says he (Lecture, p. 17), "is a drawing of a boy's head, covered with a crust *commonly known* as scald-head, the *Tinea Favosa* of medical men." *Commonly known*; and to whom so commonly known, pray, as old women? Nor do we "stand alone," as the catechist insinuates, since Dr Bennett is with us. To the second query we say Yes, frequently; and if they had as good opportunities of attending to their cases, we suspect they would succeed as often as, or maybe oftener than, those who practice t'other plan. We cannot be expected to answer the third query which, coming after the second, is *de trop* and frivolous. The catechist has not been very communicative himself, as to the speed with which the scab-triumphs of the anointers are achieved: when he gets up a quarto crammed with that point, we may possibly call a consultation of feminine antiquities, at which he may put in an appearance if he wishes 'farther particulars.' O, but the old women are not to get off so easy; they must not, they can't, they *don't* cure scald-head after all: *On Dr Bennett's authority let the statement be denied.* What! on Dr Bennett's authority? In a moment of oleaginous

furor, it seems he had forgotten his part—masked and muffled in a cloak behind the consulting room door, prompting doubtless, but not intending to be *seen* at the elbow of Mr Glen. Well, his authority is on the wane, and we respectfully return him his denial, without any qualification.

Mr Glen next considers what after his classification is Dr Bennett's "third instance"—third instance of what? Any one else who has read the lecture, must see that it is no instance, but plainly what we have termed it,—a '*statement* of a fundamental doctrine.' As usual Mr Glen gives no quotation, he knows better: it will be found at page 20 of this pamphlet. Passing over a paragraph wherein we are likened unto "an impulsive barbarian horde" (many thanks!)—

"At last," he says—

"At last Dr M'Gilechrist collects his forces and faees the third instance. In it Dr Bennett shows that the theory of cell-formation and its conditions (thongh not THE PRIMITIVE FACT which some suppose,) is most useful already in application to treatment. What says the critic? Does he deny that it is a theory? No. That it is useful in practice? No. Yet *that* is the sole point of interest *now*. What then says the critie? He asks if Dr Bennett means to exhume a mouldering humoral pathology?"

If it can be of any use to him, Mr Glen may here have one of the denials he seems to covet. It still remains to be proved that the said cell theory "is most useful already in application to treatment:" its application to treatment is rather fanciful. We did not ask the question, but rather expressed astonishment, that Dr Bennett should, as his words seem to imply, pursue the ancient humoral pathology; a march so opposite to the direction of his peroration. Mr Glen adds, that—"The enquiry is palpably absurd:" perhaps so, but then it is no fault of ours. He goes on:—

"But as the question is further irrelevant, it may be at once, on this ground, set aside; while *there*, unanswered and triumphant, stands Dr Bennett's third instance. From that point unassailed any more by critie, Dr Bennett rehearses his list of the treatment of growths, abscesses, pneumonia, pleurisy, and eaneer, and claims as proper triumphis the beneficial changes in the treatment of apoplexy, syphilis, small-pox, phthisis, and Bright's disease. Therefrom, justly, he infers by induction that great is the influence of medical science."

Now, had Dr Bennett done something more than 'rehearse his list,'—had he honestly attempted to show that:—"The beneficial changes which have taken place in our treatment of apoplexy, syphilis, small-pox, Bright's disease, and many other disorders, might be shown either to have originated from, or to be capable of being satisfactorily explained by an advanced knowledge of physiology,"—had he done this, it might have been somewhat to the purpose. But the above paragraph, from page 19 of his lecture, is all he says about it. "Therefore, justly," *does*

he "infer by induction that great is the influence of medical science? Not very *justly*, we must persist in thinking. Mr Glen continues:—

"But that influence, he acknowledges, is imperfect and capable of indefinite increase.* One reason is obvious: the science itself is inexact; all vital phenomena are not yet reduced to law, and our interference with such phenomena cannot be scientific. While the main reason is that practical men, whose business and in whose power chiefly it is to apply truth, are not acquainted, as they might be, with the laws already determined, and do not make use, as they might do, of the appliances which require to be employed during the application of these laws. Thus, " notwithstanding the universality with which the stethoscope and auscultation are now received as necessary means of diagnosis, how few of our medical men, comparatively, are really skilful in detecting by them the morbid changes going on in the heart and lungs."—(Dr Bennett, p. 19.) In regard to the explanation of the limited influence of science we again confront the critic. For, when Dr Bennett declares that few, comparatively, are really skilful, Dr M'Gilechrist remarks that they never were more numerous; and whereas Dr Bennett states there are few really skilful, Dr M'Gilechrist, as if in proof, quotes Dr Henderson's authority to show, that *of these few, some are known to have occasionally failed.*"

No; it was rather to show that "these few," who made themselves conspicuous by crying shame on a whole profession, are *the very persons* who have been most notoriously convicted at the bar of their own tribunal. They deserve what they have got. All men are mortal, and therefore prone to error; and for the modest blunderer there is ever both sympathy and hope: when he falls it is at least not far. But it is otherwise with famous presumption.

"Finally," says his advocate, "Dr Bennett looks to the future, and draws a brighter picture of the advance of science in itself, and its application by practical men. Widely different is the language of the critic." Yes, certainly, here we differ; but not as to *the advance of science in itself*, as Mr Glen would fain insinuate,—as to the probability or possibility of medicine ever becoming an exact science: *That* is the question. A question of the first consequence to the conscientious disciple of medicine; until he has settled it for himself he is building a house on an uncertain foundation. It may be pleasant, it may be popular also, to tell the flattering tale that medicine will one day become an exact science, and disease subject to fixed laws; but *is it possible?—Is it true?* Until the student has wrought out for himself the conviction that it is *not* possible, that it is *not* true, he has not, we humbly think, capacitated himself, so to speak, for aiding in the task of freeing his profession from the mists of infidelity which, now more than ever, seems to be thickening around it. Mr Glen argues that our doctrine on this subject is—"ill-timed in the present age, when the rapidity of locomotion, and the establishment of telegraphs, and the prosperity of the country under free-trade,

* The italics are ours.

and the present alliance between France and England, render us familiar with the fact that *conceivability* (Phœbns! what a word!) is no measure of truth." It is truly astonishing what these unfortunate locomotives and telegraphs, and the rest of it, are made to answer for. Could not Mr Glen get well astride of an anointed steam-engine, make Dr Bennett discharge him by electricity, and tear off to the uttermost realms of space, with fevers, choleras, coughs, and inflammations, strapped (in a syllogism) on his capacious back? That were indeed a blessed riddance!

In the remaining four or five pages of his pamphlet, Mr Glen brings against us nine charges of irrelevant statement, or "aberrations from Dr Bennett's line of argument," which he also says are incorrect: we deny their irrelevancy, and deny also that Mr Glen has proved them incorrect. He has himself so tortured and twisted the "line of argument" as to make it very difficult, for any one who should read his vindication, to determine what *both* would be at;—for if Dr Bennett did *not* mean some of the things which his language *does* convey (as Mr Glen virtually asserts), and if he *did* mean several other things which his language does *not* convey (as Mr Glen also avers), another Daniel must come to judgment in order to determine the exact value of the mess they have made of it between them.

Of the nine instances—passing over No. 1, which is a mere "marvel" at our "boldness," we take up:—2.

"2. 'The planet Neptune was calculated—it was not discovered by calculation.' (P. 8.) Here, indeed, is an important distinction in which Dr M'Gilehrist's subtlety appears in confessed power. Unfortunately there are two objections possible; this distinction is not ordinarily recognized by men of less subtle analysis, and 2d, it is so subtle that Dr M'Gilechrist himself forgets it in a foot-note to that very page, and talks like other men of Leverrier, the DISCOVERER of Neptune. How far in that passage by tacitly excluding the contested claims of our countryman, Mr Adams, to a share in the honour, he yields to an anti-English feeling, I shall leave you, Gentlemen, to consider. It may be only an oversight, but as another instance of the same apparent fondness for arbitration and partiality in the assignment of honour, I must allude to his decision (page 14) against Dr Bennett's claims as the introducee into this country, of the cod-liver oil treatment in phthisis."

Had Mr Glen quoted our words fully and fairly (he seldom or never does), it would have appeared that we drew a very just distinction between the *observation of the star* (the first and necessary proof of its existence as a heavenly body, whether as fixed star or planet), and the *discovery of the fact* that it was a planet; stating in a note that the former was made by Lalande, and there also styling Leverrier, not its "DISCOVERER" but, its discoverer *as a planet*. In this edition we put the qualifying words in italics, to give Mr Glen the chance of seeing them if, peradventure, he *can* manage to open his eyes wider than usual. We have no objection to allow that Mr Adams is entitled to *some* share in Leverrier's honour; but if one happens to think that the latter has the

advantage of our countryman—the general opinion in America as well as on the Continent—is there nothing else for it but that one must do violence to one's conscience to please Mr Glen! As to taking up the cudgels for Dr Bennett in his dispute with Dr Williams about cod-liver oil, we humbly submit that it is more than he should expect of *us*—clean out of the question. Let them fight it out like school-boys. First big boy may say to second big boy—‘ You're a lie! ’ to which second big boy may retort—‘ And you're another! ’—but as long as they don't draw their penknives on each other, the bystanders needn't interfere.

Of No. 3, it is enough to say that, speaking generally, the *fact* is as we have stated it: in practice in this country sulphur ointment is fallen back upon in serious cases of scabies. And those who havn't spent all their professional lives in an hospital know, that the chief application of the Stavesacre ointment is for the destruction of *lice*, not *aceri*.

At No. 4 (“Dr Bennett nowhere denies that *fevers* have changed their type”), the *M.A.* certainly has us on the hip. We have got it this time. But, let the conqueror spare the vanquished!

No. 5 we quote entire:—

“Dr M'Gilchrist respectfully asks, ‘What led to the abandonment of blood-letting in pneumonia?’ but, instead of waiting for an answer as one would have expected from a respectful inquirer, he appears goaded on by a longing to fix Dr Bennett between the two horns of a dilemma, and volunteers a reply of his own. (P. 19.) ‘Either, he says, ‘it was blind experience, or a theoretical demonstration of the *modus operandi* of venesection. Let Dr Bennett take his choice.’ But really Dr Bennett must decline: for, in truth, that which led to the change (*at least in Germany**) was neither blind experience, nor a theoretical demonstration of the *modus operandi* of venesection, but the homœopathic theory. Believing in this theory, Dr Fleischmann of the Homœopathic Hospital, Vienna, treated all pneumonias by globules of the appropriate remedy, and his patients recovered more quickly and thoroughly than in the bleeding establishment. But a medical bystander (Dr Joseph Skoda), observing and admitting the fact of recovery, formed another theory, that the cause was not a homœopathic dose but the power of nature, in the absence of interference by bleeding. So he gathered cases, and tested the power of nature without globules and without bleeding, with this result, that his patients recovered as well as the homœopathic, and, like them, better than those subjected to venesection.”

This is merely a story; but allowing it to be a true one, if it proves anything at all, it proves that it *was* experience that led to the comparative abandonment of blood-letting in pneumonia; for it seems he [Dr Skoda] *gathered cases, and tested the power of nature*.

No. 6 we also quote:—

“Theoretical medicine will stand silent, or retreat apalled before the next visitation of cholera.” (P. 16.) Here again is a doleful prophecy;

* The italics are ours.

but it will prove false. Theoretical medicine will not stand silent. It has *already* spoken with such authority as to procure the timely interference of the Board of Health, and with such remarkable success that the last visit, *terrible as it appeared beforehand to the imagination of empirical practitioners,** was stricken, on its arrival, of many horrors and reduced to definite proportions. Neither did theoretical medicine retreat apalled. *E contra*, scientific men were foremost in exposing themselves, and busiest in the toilsome labour of detecting the nature of the disease and the manner of its propagation."

What! was it *Theoretical* Medicine that procured the interference of the Board of Health? Had despised Observation and Experience nothing to do with it? If theory could have done it, the towns and hamlets of Scotland, still undrained (for the Bill has not passed yet), would surely have been purified long ago. And if Theoretical Medicine did not at last retreat apalled before the late visitation of Cholera, she showed great want of modesty and shame. For did not every remedy she so confidently suggested fail notoriously?—calomel and opium, on the secretion theory of the disease; salines, on the blood theory; castor oil, on another theory; and so on. O, but theoretical medicine did not retreat, because "scientific men were foremost in exposing themselves." Are *theoretical* and *scientific* to be for ever thus confounded? No; fortunately the difference is beginning to be appreciated.

7thly, Mr Glen doth feel sore because we styled Aselepoïdes 'a more famous physician than Dr Bennett'—Let him feel sore.

8thly, Mr Glen says—"that cancer is a disease of the blood, is not a fact, but a theory." O, yes, of course it is;—of course everything is a theory, for the matter of that, if it pleases Mr Glen to have it so. Only, if we mistake not, it is a *fact* that Dr Bennett believes cancer to be a disease of the blood, and that is all we require at p. 22.

9thly and lastly:—

"Dr Bennett has embraced the ancient humoral pathology."—(P.22.) The statement is doubly incorrect. Dr Bennett has not embraced any humoral pathology as a pathology sufficient to explain all the phenomena of disease; and 2d, If he has embraced any humoral pathology as explaining some groups of phenomena in disease, it is not the exhausted nor exploded humoral pathology of the ancients, but the modern theory, founded on more extended and accurate observations. The name appears to deceive the critic; because both are "humoral," he thinks that to embrace the one is to embrace the other; though the one is the first meagre generalization, and the other is the last full and *scientific* (!) elucidation."

By the term *ancient humoral pathology* we simply meant to indicate that the doctrine itself is a very old one: we did not assert—as Mr Glen affects to believe—that because Dr Bennett has embraced, or, to use his own expression, *arrived at* humoralism, his must necessarily be the iden-

* The italics are ours.

tical doctrine, in all respects, held a century ago. Dr Bennett's words are—" and so we again arrive at a species of humoralism." Without forgetting that it is but *a species* of humoralism, it was the circumstance of a *return* to humoralism—(humoralism, which, from being once the reigning dogma in medicine fell into oblivion, was exploded and lay dormant for a very long time)—that we particularly pointed to, as suggestive—to minds of such insignificant caliber as our own—of the suspicion that medicine does not progress directly, but rather makes what advances it is capable of making in a kind of orbital path. This may be nothing better than a silly theory of ours, as Mr Glen indeed insinuates plainly enough ;—but then theories are fine things in medicine, and it is a little hard that we should be grndged one out of the great dice-box, so full of them, which Mr Glen has just been rattling. What ! not one lollipop out of that beautiful basket of yours ! No : You won't ?—then don't.

Finally, Mr Glen compares Medicine to a mighty—advancing stream. Alas ! it is, we fear, still a mighty dark one ; and Dr Bennett and his *hazy* satellite are not, seemingly, the gifted luminaries destined to shed on it much lasting light.



